

LIFE AMONG THE SCIENTISTS

An Anthropological Study of an
Australian Scientific Community

MAX CHARLESWORTH
LYNDSAY FARRALL
TERRY STOKES
DAVID TURNBULL



OXFORD
UNIVERSITY PRESS

Melbourne
Oxford Auckland New York

part of a general overall science of biological information and control—a new ‘general biology’, the centrepiece of which is the genetic code.²⁴ As one of the participants puts it: ‘DNA says it all’.

A number of the symposium participants comment upon the fact that before the clonal selection theory immunologists could be classified as either chemists or biologists, but that the theory had brought about a marriage of these elements such that no self-respecting immunologist could ignore either. Ironically, of course, Burnet had done just that. According to Nossal, Burnet didn’t trust chemists; he ‘shunned’ them and ‘made an enemy of chemistry and chemists’. Eisen (from Massachusetts Institute of Technology) notes that a new book with the title *Molecular Cell Biology* exemplified what would have been impossible in the 1950s and early 1960s—a commitment to a joint biological and chemical approach in immunology.²⁵

What emerges from a number of the papers at the Toronto Symposium is that, while the acceptance of the clonal selection theory and its role in furnishing a theoretical framework for a ‘mature’ immunology provided a triumphant finale to Burnet’s career, his younger collaborators and successors at the Institute were much better placed than he was to capitalize on the new directions in immunology. Burnet’s rather strange antagonism to chemistry and to high technology in science made him unsympathetic to the combination of biology and chemistry that his theory helped bring about. It also made him suspicious of the technique-dominated biology that was emerging in the 1960s. Like a scientific Moses, he was not to enter the Promised Land to which he had led others.

Nossal

Nossal, Burnet’s successor, was fortunate in that he had another mentor in Joshua Lederberg whose commitment to the new biology and to high technology in science provided him with an entirely different model from that espoused by Burnet. Lederberg was visiting the Institute when Nossal began his work and it was to Lederberg’s laboratory at Stanford that Nossal went while he was still working on his one cell/one antibody project which was important in the clonal selection theory gaining acceptance.

Nossal was Burnet’s own choice of heir-apparent as Director of the Institute (the story is that Nossal was the only applicant for the

post) and there is, *prima facie*, some cause for puzzlement about this since their personalities and scientific outlooks were poles apart. The older Burnet was austere and reserved while the much younger Nossal (he became Director at the age of thirty-five) was enormously vital and enthusiastic and extroverted. Again, Burnet was committed to small and individualistic science using modest resources and eschewing elaborate scientific technology, while Nossal on the other hand embraced big science and recognized the need for group research and new scientific technology.

One might have thought that Burnet would have found Nossal’s personality and scientific style completely antithetical to his own. That, on the contrary, he selected and designated Nossal as his successor reflects credit on Burnet. Of course, Nossal had, through his own research, vindicated Burnet’s clonal selection theory, and then again he had the powerful support of Joshua Lederberg who was then beginning his career as a scientific entrepreneur and power broker.

Burnet and Gajdusek

A number of those who knew Burnet personally give the impression that he was a rather difficult character, very conscious of his own worth as a scientist and impatient with lesser mortals. Again, despite his carefully cultivated apolitical stance, he also favoured conservative doctrines such as eugenics. On my return to the Institute after the Toronto Symposium I talk to Stephens (a former member of the Institute) about the great man. He says that a revealing aspect of Burnet’s scientific personality is shown in his dealings with the romantic and extraordinary American scientist, D. Carleton Gajdusek, in the late 1950s. Gajdusek had studied with Linus Pauling and Max Delbrück at the California Institute of Technology, and John Enders at Harvard (he used to joke that everyone he studied with eventually got the Nobel Prize!) and in 1957 he came to visit Burnet at the Institute. Gajdusek was a flamboyant character with an extraordinary range of interests, but though Burnet was impressed with his work he did not consider that he was a first-rate scientist.

After eighteen months at the Institute Gajdusek visited Papua New Guinea and there became interested in the mysterious disease called kuru which afflicted the Highlands people. Burnet had also been interested in kuru and viewed the investigation of the disease as an Australian project. When Gajdusek made known his designs to take on kuru as his own research project Burnet saw him as an

Biomedical research institutes can respond to advances in theory and technique by quite radically altering their focus. The Hall Institute provides a good example of this. During the years following the Second World War, almost everyone there was engaged in one way or another in studying the biology of influenza. But Macfarlane Burnet was also developing an interest in immunology. By 1958, the Institute had become almost entirely in immunological research institute.

This also illustrates the peculiarly influential role of Directors in scientific institutes. It is not only a question of power, but also once again one of scale. Universities are simply too big for their Vice-Chancellors to completely reorient them single-handed. On the other hand, a charismatic Director is able to turn an Institute around. For example, in 1957, when Hilary Koprowski came to the Wistar Institute of Anatomy and Biology in Philadelphia as its new Director, it was moribund. The large collection of skeletons gathering dust and cobwebs meant that the dead outnumbered the living. With Koprowski came a half-dozen young scientists he had persuaded to help him revitalize the place. The skeletons were sold and the space they occupied filled with laboratories. Anatomy was replaced by study of viral degenerative and malignant disease. By 1984, a total of 300-odd scientific and support staff consumed a budget of more than US\$17 million—80 per cent of it derived from competitive grant awards. The Wistar is very much Koprowski's Institute. He created it.

The biomedical research institutes and their directors are, in some ways, an exclusive international club. Whilst I was at the Wistar, the Director of the Basel Institute of Immunology, Fritz Melchers, was also present, taking an annual break from his duties to do some hands-on research. The Director of the International Institute of Cellular and Molecular Pathology in Brussels, Christian de Duve, is also a full Professor at the Rockefeller University in New York. (The Rockefeller Institute for Medical Research was the first medical research institute in the US, having been founded in 1901. The change from Institute to University took place in 1965, in recognition of its substantial, but subsidiary role in training Ph. D. students.) The President of Rockefeller, Joshua Lederberg, was at Stanford when I visited, doing research. Directors of biomedical research institutes all serve from time to time on each other's governing boards and scientific advisory councils.

One could easily multiply examples of the central importance of these directors. The Swiss Basel Institute, established in 1968 by the

giant pharmaceutical firm Hoffman-La Roche, is an institute for immunology because its founding Director, Niels Jerne, was an immunologist. Jerne was awarded a share of the 1984 Nobel Prize for Physiology or Medicine for his theories concerning immunological specificity in the development and control of the immune system.

The Basel Institute is unusual in that most of its scientific staff are either visiting on sabbatical leave from their permanent institutions, or are rising young stars on non-renewable short-term contracts. As a result many Hall Institute immunologists spend time in Basel. The two places are of a roughly comparable size, too—which makes them both considerably smaller than most of their peer group of leading international biomedical research institutes. Consequently, they share an institute-level *esprit de corps*, having common rituals like eating lunch and drinking morning and afternoon tea together. Every scientist at the Basel Institute has a personal technician assigned to assist her or him. In this, too, the Basel and Hall Institutes are similar.

Most biomedical research institutes have some feature or other which distinguishes them from all, or most, others. At Basel, it is the sharp focus on immunology, the short stay of most scientific staff, and the generous private funding by Roche—which means that no one need ever make a grant application. Other institutes also achieve this freedom from outside granting agencies, but in different ways. At the Imperial Cancer Research Fund (ICRF) Laboratory in London, for example, all the research is financed by donations. Cancer research is comparatively easily supported because the public are far more worried about cancer than, say, heart disease—even though they are much more likely to die of heart disease than cancer. Thus, in 1984, the ICRF had an income of more than £24 million, almost all from legacies and donations.

The world's richest granting agency, the US National Institutes of Health, has its own internal research program, mostly on-site in the suburbs of Washington, DC. Ironically, those who work in this 'intra-mural program' are directly funded and so do not have to apply for grants. This is reminiscent of the John Curtin School of Medical Research at the Australian National University which, like the ANU itself, is funded directly by the Federal Government. This precludes them from applying for grant funds, exceptional circumstances apart.

There are some important organizational contrasts between the Hall Institute and US biomedical research institutes, such as the Scripps and the Salk Institutes on the west coast, or the Sloan-

6

Professionalization, Gender, Ethics

A certain kind of anthropology can be written in such abstract terms as interaction, structure, culture, relation, function and so forth, after a manner in which only faint significations are allowed to come from the real world. But it can well be left to the very young, the very old and the very brilliant.

W. E. Stanner, *On the Study of Aboriginal Religion*¹

Entering science

Scientists seem to drift into a scientific career without much deliberate planning or forethought. Chance and luck appear to play a large part, or so it seems later on. The French anthropologist Lévi-Strauss remarks somewhere that there is probably a reason why this anthropologist chooses such and such a tribe to study, and why that anthropologist chooses some other tribe as 'his' or 'hers'. However, none of the Institute scientists appear to think that they were predestined, either by temperament or innate talents, to take up the life sciences as against the physical sciences, or immunology as against other sections of the life sciences. Cuff, a young medico who is moving into scientific research via a Ph. D., says that in medical practice you get more or less 'immediate gratification' but that in pure science you have to learn to 'postpone gratification' since any rewards are usually a long way down the track. You need then to have that kind of predisposition for science, but that's about as far as it goes. The Director was led into immunology through the good fortune of studying with a charismatic professor at the University of Sydney and later paying a visit to the Institute when Macfarlane Burnet was director. When the Director joined the Institute in 1957, Joshua Lederberg, soon to become one of the US's leading biologists and scientific entrepreneur extraordinaire, happened to be

visiting the Institute and the Director became a protégé of his. The Assistant Director, Metcalf, had also got into research science by good luck rather than good management. Most of his medical teachers at the University of Sydney were 'fossilized' and he had learnt nothing from them. His big break had come when he was offered the chance of doing a further year of research in order to gain a B. Med. Sci. During this year he had been strongly influenced by a remarkable lecturer. This teacher had been a prisoner of war of the Russians in the Baltic States and he could not bear to be in enclosed spaces, so he insisted on lecturing with all the doors open! Though he was quite eccentric, he was also an inspiring teacher and Metcalf caught his enthusiasm for pure science.

Scientists come to the Institute both from the medical side and the scientific side. A good deal of criticism is expressed about the inadequacy of both medical and scientific training at the university level and only one or two Australian university departments in biology are thought to be of any value. Randall says he thinks that a good many potential research scientists are put off by the lousy teaching they get at university and by the old-fashioned 'disciplinary' approach to biology that still prevails.

Could recruitment into research science be made less haphazard? Galway does not think so. Really, it's just like entry into any other profession—law, engineering, the church, or even entering into marriage. As a Ph. D. student you need to be lucky with your supervisor and with your topic; you can easily end in a cul-de-sac and find yourself, after several years' hard work, up the proverbial creek without a paddle. The same is true of training overseas: it's important to have a powerful patron and to be in a place where things are happening. Scientific staff at the Institute are recruited by a kind of grape-vine process. You know that so and so is a bright up-and-coming young scientist and you keep your eye on him or her and check him or her out through friends and colleagues in the network. Young scientists have to make their run between roughly twenty-five, when they finish their Ph. D., and thirty or thirty-two, by which time they will have had experience overseas, have produced a couple of significant papers, and generally become known as 'promising'. The scientist's training is therefore an intense ten or twelve year process of formation: four or five years for a first degree (in science or medicine), two or three years for the Ph. D., then three or four years overseas or other experience. All that time you have to be proving yourself and making good and above all showing that you can attract funding.

immunology such as organ transplantation, autoimmune diseases and cancer. That's where the drive was going. There was really very little drive going into new or improved vaccines, least of all Third World vaccines. So I wrote fairly extensively at the time about our need to increase our profile in that area.'

'Where did you go from there?' I asked the Director.

'I think that that 1972 talk and my friendship with Goodman were central, but in 1973 I joined WHO's Advisory Committee on Medical Research (ACMR): this is the global body which guides WHO in all its research endeavours. It is now known as the Advisory Committee for Health Research—ACHR. There were four or five of us on the committee who saw a really great opportunity. These included Jacques Monod, the great bacterial physiologist and Nobel Laureate who was then director of the Pasteur Institute in Paris, Josh Lederberg who was my former teacher and probably then still at Stanford (he was to move later to Rockefeller University) and who is one of the doyens of American science, and Christian de Duve, a Belgian who'd recently founded a large new medical research institute and who also was to get a Nobel Prize a few years later, and myself. I think they were the archplotters, together with Howard Goodman from WHO in the background, who said in effect that it was now time to do something serious. We were still thinking in fairly vague terms about the new biology, which was crystallizing around three disciplines—genetics, molecular biology, and immunology focusing on Third World diseases. But it became apparent that something as woolly as that wouldn't run. The idea gradually gelled in discussions that maybe parasitic diseases would be the field to which these disciplines could be applied. To be frank, I don't think anyone can tell who had the idea that it shouldn't be an expanded unit of immunology, cell biology, molecular biology and genetics—which was the first rubric under which we were working—but that it should be especially focused on parasitic diseases. It may even have been a decision from the WHO secretariat.

'Anyhow, the next year something went badly wrong, which is quite amusing. It was round about 1974. The ACMR had written a paper about this new initiative. The Deputy Director-General of the organization was a chap called Lambo, a very brilliant and very personable Nigerian. He used to be a psychiatrist and after that was Vice-Chancellor of the University of Ibadan. Anyhow, he was going on a State visit to Zambia and like many Nigerians he's rather impulsive. He's a man with a great sense of humour, a wide open

ebullient personality. He was greeted by President Kenneth Kaunda of Zambia and taken to the second largest town called Ndola, one of the copper-belt towns way in the back blocks. There was a magnificent hospital there, as big as the Royal Melbourne Hospital, entirely empty. Kaunda said to Lambo, "What an ideal place for your Institute of tropical diseases". So the idea was born in Lambo's mind that a large institute in the middle of Zambia should be where all this research was to be done. I was asked to go and have a look at it the next year, 1975.'

'A delicate piece of scientific diplomacy', I observe.

'It soon became quite apparent that that idea could not possibly run. You might have a sporting chance of creating an institute in Nairobi, but putting it in a place where there were daily shortages of bread and paper—let alone test tubes and petri dishes—and where the communication linkages with the rest of the world were negligible, meant that it had no chance. However, on the visit there (to which a number of people went including Goodman, a Nigerian scientist called Lucas, myself and one or two others) we had to report back that it was no go. But we did see that you could use it as a centre for clinical studies, epidemiology and field trials if you could get the advances made elsewhere in the world. So we rescued something of the WHO Deputy Director-General's dignity and there is now a little clinical epidemiology unit there. I'm dwelling on this point because it's absolutely critical to the rest of the program—remember the name Lucas, public health professor from Ibadan, Nigeria.

'With the failure of the idea of the *Zambian Institute* came the idea of a paper institute, with its own scientific working groups, planning functions, grant-giving functions, and it was this idea that was proposed to the ACMR in 1975. The model was similar to that of a WHO scientist called Kessler who had started a small but quite effective human reproduction program with about US\$10 million a year. This body gave grants to developed countries, created training centres in developing countries and tried to promote research on birth control.'

The Director continues. 'Now we come to my own part. I had planned to spend 1976 on sabbatical leave at the National Institutes of Health in the US and get back to the lab a bit more. I'd been offered one of these Fogarty International fellowships to live in splendour in the 'stone house' on the campus. Plans were not advanced (the Fogarty International Centre is in the NIH in Bethesda) but were being made. I was to have worked in the immunology